

A COMPARISON WITH THE BAROMETRIC STANDARD OF COSTA RICA.

In a letter of August 13 Mr. H. Pittier states that he had lately sent one of his assistants to Limon, Costa Rica, with a new barometer that had been carefully compared with the standard of the Physico-Geographical Institute. The mean result of ten observations taken hourly during two days (July 29 and 30) showed that the barometer at Limon read too high by 6.54 mm., and that this error had existed for several months. The instrument is an old French model with a broad cistern and Fortin's adjustment for transportation. The error was largely due to the fact that before being transferred from one room to another, several months ago, the cistern had been properly screwed up, but had not afterwards been completely screwed down. Even after the latter operation had been properly performed on July 29 by the assistant, there was still need of a correction of 0.6 mm. Consequently, the faulty barometer was brought back to Costa Rica and the other one left in its place; this latter was constructed by James Green, of New York, and left at San Jose by William Gabb, but subsequently repaired by Negretti and Zambra; its readings may be relied on.

The international comparison of the barometric standards of various European countries and the United States, executed by Prof. Frank Waldo in 1885, needs now to be renewed and extended to include all the American States. General meteorological studies require that the pressure should be known to within 0.01 inch at all important stations in the Northern and Southern Hemispheres.—C. A.

PHYSICS AND METEOROLOGY.

A recent letter from a correspondent says:

In the course of two or three years teaching of physical geography, etc., many questions have arisen, for some of which I have found no satisfactory or authoritative answers. I do not know whether it is your custom to answer such questions either privately or through the columns of the REVIEW, but I take the liberty of submitting the following queries:

1. What effect, if any, does the increasing density of the earth's atmosphere, near the earth's surface, have upon the amount of insolation absorbed by the atmosphere? I do not find this given in the meteorologies as a cause for unequal heating of different strata of air. I have wondered if it has any effect.

2. Ganot's Physics, page 412, says that ordinary undried, but not especially moist, air was found in a certain experiment to absorb 72 times more heat than dry air. Davis's Meteorology says that water vapor is found by experiment to be as poor an absorber as dry air. Are not these statements contradictions and which is correct? To what experiments does Davis refer?

3. Is there any clear explanation why increasing the temperature of a space increases the capacity of that space for vapor? If I understand correctly, a space becomes saturated when the vapor pressure becomes such that for every molecule of water forced into it from the evaporating surface one is forced out. When this equilibrium is attained would it not seem that raising the temperature of the vapor in the space would, by causing an increase in the vibration of the vapor molecules, cause a greater expansive force and thus prevent rather than allow more vapor to pass in? Of course experiment proves that more will pass in and can exist in the vapor state when the temperature is raised, but is there any explanation for it?

4. Can the article published in the REVIEW some time ago on the "Gulf Stream Myth" be regarded as authoritative? Of course the influence of the stream has been overestimated, but does not this article underestimate its effect? Davis seems to think the ocean currents are important enough to attribute to them the cause of the deflection of the isotherms. Is he correct in this or has he underestimated the influence of the winds?

The following replies are published as being of general interest:

1. In reply to the first question, it may be stated that the amount of heat absorbed by a layer of gas of given thickness is proportional to the thickness of the layer and its transparency; the latter depends on density and dust or haze, therefore a layer of air one foot thick absorbs more when near the earth's surface than when high up in the atmosphere. But

for dustless air this amount is so small that it will not account for the unequal warmth of the different strata of air, since clean, dry air is exceedingly diathermanous.

The following quotation from Prof. F. W. Very's article on the "Solar Constant" will, perhaps, elucidate this subject:

It is commonly supposed that the larger portion of the heat produced by the absorption of the solar rays remains in the lower layers of the atmosphere, because these are richest in the vapor of water and in dust. See, for example, M. Crova's *Mesure de l'intensité calorifique des radiations solaires et de leur absorption par l'atmosphère terrestre*, p. 1, Paris, 1876. M. Radau, *Actinométrie*, p. 12, says: "In proportion as the rays penetrate into the atmosphere they encounter layers more and more dense, and the loss which they experience through unit path is proportional: (1) to the actual intensity of the beam; (2) to the density of the layer which they traverse; (3) to a constant coefficient of absorption * * * which varies with the nature of the rays." On page 14 Radau says: "The absorption is due in great part to the vapor of water distributed in the lower layers of the atmosphere." Although it is recognized (page 18) from the observations of Desains that the ratio of long-wave solar radiations on a high mountain to those at sea level must diminish when the air is very moist, nevertheless no objection is made to the use of formulæ in which the aqueous component of the absorption is assumed to be proportional to the density of the aqueous vapor.

The actual case is much more complicated. Selective reflection increases in the lower atmospheric layers, but does not warm them. Low layers of a moist atmosphere become hot because they absorb the rays of extremely long wave-length emitted by the heated soil. The sun heats these layers indirectly by first heating the ground, but contributes little heat directly, since the rays absorbable by aqueous vapor have been nearly all sifted out of the sunbeam before this reaches the lower atmospheric layers. On the other hand, the higher atmosphere, which contains a smaller quantity of aqueous vapor, is the first to attack the incoming rays. It is in the upper layers that the aqueous absorption of the solar infra-red rays takes place chiefly, and these are therefore the layers which are most warmed by the direct rays of the sun. I have noted elsewhere (*Atmospheric Radiation*, p. 123) that after rising above the comparatively thin layer of convectionally heated air, that portion of the diurnal range of temperature due to the immediate absorption of the solar rays may be expected to increase up to nearly the limit of the aqueous atmosphere, and it is surmised that this variation may possibly approach a 10-fold ratio of that which occurs at altitudes of one or two kilometers.

2. In reply to the second question experts have differed widely in their statements as to the diathermancy of aqueous vapor. Preston, in his *Theory of Heat*,² says:

Experiments of Lecher and Pernter.—More recently a series of experiments on the absorption of radiant heat by gases and vapors has been published by Ernest Lecher and Joseph Pernter,³ but these new investigations, instead of settling the question in dispute between Tyndall and Magnus as to the comparative absorptions of dry and moist air, place the whole matter in a state of greater uncertainty. For whereas Tyndall found an exceptionally low absorption for dry, and a high absorption for moist air, while Magnus found the same absorption for both, and that tolerably high, the results of the experiments of Lecher and Pernter show particularly no absorption for either, or, in other words, both dry and moist air act as a vacuum toward radiant heat.

It may be safely accepted that aqueous vapor energetically absorbs only special wave-lengths in the spectrum, but so does dry air absorb other waves. If these special waves happen to be contained in the beams of radiation on which laboratory experiments are being made the results of measurements of absorption will be quite different from measurements made on other beams that do not contain the special wave-lengths. It is quite plausible that the differences between different experimentalists and between the statements in the different works on physics are due to differences in the character of the radiations that have been experimented with. Professor Langley's work for the last twenty years has been devoted to measurements which it is hoped will clear up these discrepancies.

The quotation given above from Professor Very indicates a very considerable absorption of solar radiation by the aqueous vapor in the upper atmosphere. Professor Very's conclusions

¹ Monthly Weather Review for August, 1901, Vol. XXIX, p. 364.

² The Theory of Heat, Thomas Preston. London and New York, 1894, pp. 485-486.

³ Lecher and Pernter. Sitzb. der k. Akad. der Wissenschaft in Wien, Juli, 1880; Phil. Mag., January, 1881.

are based largely upon the study of the absorption bands of the solar spectrum, as measured by means of Professor Langley's bolograph, and are undoubtedly entitled to great weight.

There may be a question, however, as to the state in which moisture exists in the upper layers of the atmosphere. On account of the low temperature at great heights the amount of moisture that can exist in a state of vapor is very small, and it may be that moisture in the form of minute solid particles also exists. The action of these latter would be quite different from the absorption by water vapor.

3. As to the third question, any satisfactory explanation of the fact that the capacity of space for vapor increases with the temperature of the space, or what is the same thing, the temperature of the vapor, must depend upon our knowledge of the nature of heat and molecules. According to the commonly accepted mechanical theory of heat and the kinetic theory of gases, the heat contained within a mass of vapor is simply the sum total of the kinetic energy of the rapidly moving molecules of vapor. The molecules of a gas are supposed to be far apart, relative to their own size, but by moving rapidly, by rebounding against each other and against the boundary of the enclosure, they occupy or dominate a large volume. To increase the temperature of this enclosure is to increase the velocity of these molecules and therefore the number of impacts and reflections that occur per second.

By increasing the velocity and therefore the kinetic energy of the molecules, we also increase the momentum with which they strike each other and the boundary surface, that is to say, we increase the general expansive pressure of the vapor. If no liquid is present and therefore if no evaporation throws more vapor into the space, then this increase of pressure corresponds to that due to the ordinary coefficient of expansion of the gas; but if we allow heat to break up liquid molecules into gaseous ones, and evaporation to increase the number of molecules in the gaseous space, until it is saturated at the new higher temperature, then the increase of pressure is the sum of two causes, namely, the increased momentum of the old molecules, and the added momentum of the new ones.

We have not used the term "vibration of vapor molecules," because that implies some regularity like an oscillation, but we speak only of the free movements and impacts of the molecules. The free movements are presumably always in nearly straight lines, as the molecules pass from one impact to the next, but the total path of a molecule is a broken line. The kinetic theory of gases allows us to determine approximately the average velocity of the molecule and the length of the average, or mean free path between two successive impacts in each kind of gas.

In order that our correspondent and readers may have the choice of several methods of looking at this question, so as to adapt their teaching to the needs of the various grades of students, we give the following extracts from recent correspondence:

Under date of April 4, 1902, Prof. J. S. Ames, of Johns Hopkins University, Baltimore, Md., writes as follows:

My understanding of the reason why an increase in temperature increases the evaporation of a liquid has always been along the lines that the effect of temperature on the liquid is such as to so increase the velocity of the particles that more of them are able to escape from the surface, and that therefore the evaporation is increased. At the same time, of course, there is an increase in the velocity of the particles of vapor. But the question as to these particles reaching the liquid surface is more one of the mean free path than of anything else, and this is not affected to any such extent as to increase the rate of condensation to as great an extent as the evaporation.

Under date of April 15, Prof. J. Willard Gibbs, of Yale University, writes, as follows:

In regard to your correspondent's question, we must remember that the average velocity of molecules in the liquid is increased as much as

the average velocity in the vapor (when the temperature is increased). The restraining power of the attractions in the liquid will evidently have less effect in these greater velocities.

Dr. Edgar Buckingham, now physicist in the Department of Agriculture, under date of May 10, 1902, writes, as follows:

Heating a gas or vapor in an enclosed space of fixed volume increases its pressure. If we accept the hypothesis that matter is made up of molecules, or separate particles, we account for this increase of pressure by saying that the energy, put into the vapor in the form of heat to raise its temperature, has gone, at least in part, to increasing the kinetic energy of translation of the molecules, so that when the vapor is hotter its molecules fly about more violently, strike harder and oftener on the walls of the enclosing vessel, and so exert a greater pressure.

Now, suppose that a part of the enclosing wall consists of the surface of the liquid from which the vapor has been sent off. If we raise the temperature of the vapor without raising that of the liquid, we can not have a state of equilibrium, and we can not speak of a definite pressure of the vapor, or of a definite vapor density; we must always keep the liquid and the vapor at the same temperature and imagine them heated or cooled simultaneously.

Suppose then, that we heat a liquid in contact with its vapor, the two being enclosed in an envelope of invariable volume. According to the kinetic hypothesis, the vapor molecules fly about more violently; they strike the surface of the liquid harder and oftener; and we should naturally expect more of them to get caught among the molecules of the liquid, so that the vapor density would decrease, and we should have, in effect, a condensation of vapor and a decrease in the vapor density.

But we have been considering only the vapor without paying any attention to the liquid, and this upsets our former conclusion. It is true that more molecules of vapor may get caught in the liquid, and so be condensed, or become a part of it. But, on the other hand, the kinetic hypothesis assumes that the molecules of a liquid also are in a state of motion, although they move through shorter distances, and with less freedom than the molecules in the vapor. Heat applied to raising the temperature of the liquid, increases the violence of motion of the molecules of the liquid too. Hence, more particles are likely to arrive at the surface of the liquid, from within it, with sufficient velocity to tear themselves away from the attraction of their fellow molecules and fly away freely into the space filled with the vapor.

There are thus two opposing tendencies; one for the molecules of the liquid to fly off into the vapor space, and the other for the molecules of the vapor to get entangled in the liquid. A state of equilibrium is reached at any given temperature, when the effects of these two tendencies just balance each other. On the kinetic hypothesis, if we raise the temperature of the liquid and its vapor, both these tendencies are increased. Whichever is increased most will then predominate. But there is no way of seeing a priori, why the one tendency should increase faster than the other, because we do not know enough about the internal structure of liquids, or the way in which the mutual attraction of their molecules is influenced by temperature. We may, to be sure, say that we know that the cohesion of liquids decreases as the temperature rises, because we know experimentally, that the surface tension (which is an expression of this cohesion) does decrease with rising temperature. Hence, we may say that, with rising temperature, the violence of motion of the molecules in both liquid and vapor increases, but the restraining attractions in the liquid decrease, and there is nothing on the side of the vapor to offset this. And so, on the whole, we might expect just what actually happens. It is just as in many other cases; if we know all about the facts, we can predict some of them from the others; but we should not have been clever enough to make our prediction, if we had not known what it was that had to be predicted. Practically, we have here to content ourselves with the mere statement of the fact, namely, that the effect of the tendency of the vapor molecules to tear themselves free, by reason of their increased velocity at a higher temperature, invariably does increase faster than the effect of the tendency of the vapor molecules to fly back and become a part of the liquid; so that we invariably do have an increase of the vapor density with a rise of temperature.

The important thing is always the fact. Everyone who goes any distance in physics comes to realize that there is, in reality, no such thing as an "explanation" of anything; that the object of physics is not to "explain" facts, but to get them organized, formulated and coordinated by as few simple general statements or so-called laws as possible. The kinetic theory of gases is a good example of explaining simple and familiar facts by means of a difficult hypothesis; it assumes the existence of molecules which no one has ever seen and of which it is difficult, if not quite impossible, to form any clear conception reconcilable with all the facts. We "explain" the familiar facts of evaporation by referring them back to molecular motions, which we know nothing about from direct observation.

Under date of May 27, 1902, Prof. Ernest Merritt, of Cornell University, Ithaca, N. Y., writes, as follows:

To make the point raised by your correspondent perfectly definite, let us suppose that a certain quantity of water is placed in a closed vessel,

the space above the water containing saturated vapor. The presence of air would, of course, not modify the conditions essentially. If the temperature of the whole mass is raised it is a matter of observation that some of the water evaporates and the vapor becomes more dense, yet the pressure of the vapor is increased, and therefore the tendency of vapor molecules to go back into the liquid is greater than at the lower temperature. From the standpoint of the kinetic theory I think the explanation is somewhat as follows:

Owing to the rapid motion of the molecules some of the more rapidly moving water molecules are continually escaping from the attraction of their neighbors and passing out into the vapor, while some of the vapor molecules are at the same time continually returning again to the liquid. When these two processes just balance one another the vapor is said to be saturated. Now, if the temperature is raised the motions of the molecules become more rapid. This is true, not merely in the vapor, where their increased speed leads to greater pressure, but also in the liquid, where the result is an increased tendency for molecules to escape into the vapor. More molecules return to the liquid each second than before, but more molecules also leave the liquid each second. At first glance it is impossible to tell which of these two opposing tendencies will prevail: whether the water will evaporate or the vapor condense. A closer consideration shows, however, that the former is what should be expected. In the liquid the rise of temperature produces two effects: it increases the average speed of the molecules and it diminishes the average attractive forces between them. Just why this latter effect should be produced could only be "explained" by some more elaborate and detailed theory of molecular forces than now exists. But there can be little doubt of the fact. (Consider, for example, the effect of temperature on tensile strength.) In the vapor the molecules are so far apart that their attractive forces have very small influence upon the motion. The tendency for molecules to return to the liquid from the vapor therefore increases with rise in temperature less rapidly than the tendency of liquid molecules to escape. The result is that the density of the saturated vapor increases with the temperature.

If preferred, the matter may be put in analytical form. Whether anything is gained by so doing in this case is largely a question of individual opinion.

Suppose that a vessel contains water and water vapor in equilibrium, i. e., the vapor is just saturated. The condition for equilibrium is that no change in the system which leaves the pressure and temperature unaltered can cause an increase in the thermodynamic potential, Φ . If ϕ_1 and ϕ_2 are the thermodynamic potentials for a gram of water and a gram of steam, respectively, while M_1 and M_2 are the masses of water and steam, the condition may be written:

$$\delta\Phi = \delta[M_1\phi_1 + M_2\phi_2] = 0.$$

Now, $M_1 + M_2 = M = \text{a constant}$; the change represented by δ is one which does not alter ϕ_1 and ϕ_2 (since these are functions of T and p only). Therefore

$$\delta\Phi = \phi_1\delta M_1 + \phi_2\delta M_2 = 0 = \delta M_1(\phi_1 - \phi_2).$$

Suppose now that the temperature of both water and steam is raised to $T + dT$, but that the quantity of liquid and the quantity of vapor are kept unchanged. At the temperature T they were in equilibrium. Are they so still at the new temperature? If so, we should have $\delta\Phi' = 0$ where the prime refers to the new temperature; and

$$\delta\Phi' = \delta \left[M_1 \left(\phi_1 + \frac{\partial \phi_1}{\partial T} dT \right) + (M - M_1) \left(\phi_2 + \frac{\partial \phi_2}{\partial T} dT \right) \right]$$

This reduces to

$$\delta\Phi' = \delta M_1 \left(\frac{\partial \phi_1}{\partial T} - \frac{\partial \phi_2}{\partial T} \right) dT.$$

There is, however, no reason to expect that equilibrium will still be maintained. It is rather to be expected that water will be evaporated, or that steam will condense. Since Φ' can not increase, that one of these processes which makes $\delta\Phi'$ negative is the one that will occur.

Now

$$\phi_1 = w_1 + pv_1 - Ts_1$$

And

$$\phi_2 = w_2 + pv_2 - Ts_2.$$

The subscripts 1 and 2 refer to water and vapor, respectively, while w , v , and s represent the internal energy, the specific volume, and the specific entropy, respectively.

$$\begin{aligned} \delta\Phi &= \delta M_1 \frac{\partial}{\partial T} [w_1 - w_2 + p(v_1 - v_2) + T(s_1 - s_2)] dT \\ &= \delta M_1 \left[(v_1 - v_2) \left(\frac{\partial p}{\partial T} \right)_M - (s_1 - s_2) \right] dT. \end{aligned}$$

Remembering that $T(s_2 - s_1) = r$, where r is the heat of vaporization, and that

$$r = (v_2 - v_1) T \frac{\partial p}{\partial T}$$

We have

$$\delta\Phi = \delta M_1 (v_1 - v_2) \left[\left(\frac{\partial p}{\partial T} \right)_M - \frac{\partial p}{\partial T} \right] dT.$$

The derivative $\left(\frac{\partial p}{\partial T} \right)_M$ refers to the temperature rate of change of pressure for a fixed amount of vapor, which is allowed to supersaturate: $\frac{\partial p}{\partial T}$ without the brackets, gives the temperature rate of change

of the vapor pressure corresponding to saturation. Now, so far as the writer is aware, the pressure of saturated vapor always increases with the temperature more rapidly than the pressure of unsaturated vapor at

the same temperature. Therefore $\left(\frac{\partial p}{\partial T} \right)_M - \frac{\partial p}{\partial T}$ is negative. It also

seems universally true that a given mass of substance occupies more space in the vapor form than as a liquid. Therefore $v_1 - v_2$ is negative. In order to make $\delta\Phi'$ negative, we must therefore have δM_1 negative. In other words, the tendency is for water to evaporate, and the vapor becomes more dense. If some substance could be found which contracted upon evaporation, such a substance would act exactly the opposite way from the usual one which your correspondent cites. I see nothing in the preceding reasoning to exclude the possibility of such a substance being found, although it would be remarkably different in its properties from what we are accustomed to.

4. In reply to the fourth question we may say that the author of the "Gulf Stream Myth" may perhaps not be considered a technical authority in meteorology, but his article certainly seems to the Editor to be perfectly fair. We do not find any important discordance between him and Prof. William M. Davis (see page 68 of Davis's *Elementary Meteorology*). All agree that the ocean surface water gives more moisture and therefore more latent heat to the air than does the same area of land at the same latitude, but it gives less sensible heat or temperature. All agree about the general surface drift of the North Atlantic under the influence of southerly and westerly winds, and that the winds and currents combine to carry the isotherms northeastward toward Iceland and Spitzbergen; but the present question is as to the special influence of the Gulf Stream proper in deflecting general isothermal lines, in comparison with the general influence of the oceanic surface drift and the winds. On this there can be but one opinion: viz, it is insignificant. The Gulf Stream off the coast of Florida may be allowed to have a velocity of 4 miles per day and the cross section of the stream may be 5 square miles. The surface drift of the oceanic regions west of Ireland may average 1 mile per day across a line extending from southern Ireland north to Iceland, or 900 miles; the cross section of this drift has an area of perhaps 2 square miles, the drift being toward northeast. A similar drift toward the southeast prevailed between Ireland and the Azores. At an average rate of 2 miles per day, it would require fifteen hundred days for the surface waters of the Gulf Stream proper to reach the eastern Atlantic coast and turn either northeastward or southeastward. In this long time its surface temperature would be modified by alternating northerly and southerly winds and would have affected the temperature of all the air that has blown over this part of the ocean. The vapor evaporated from the great area of cool water is more important than that from the smaller area of warm Gulf Stream water. The southwest winds that bring moisture, cloud, and rain, and warmth to Europe, get far more of all these from the general surface of the Atlantic than from the Gulf Stream proper.

In the MONTHLY WEATHER REVIEW for August, 1901, page 376, is an extract from an article entitled "Popular errors in meteorology and geography," by Mr. Henry Gannett. A section is devoted to climate and ocean currents, in which he reaffirms the views of Mr. Harvey M. Watts as set forth in the "Gulf Stream Myth" (MONTHLY WEATHER REVIEW, Vol. XXVIII, page 393).

The comparatively warm climates of the western shores of Europe and America are to be attributed to the prevailing

moist winds that blow upon them from the warmer portions of the Atlantic and Pacific Oceans, respectively, and not to any abnormal degree of heat that is conveyed to those coasts through the medium of the ocean currents. To be sure the warm winds and currents appear to accompany each other, but no doubt the currents are more dependent upon the winds for their strength than are the winds upon the currents for their temperature.

METEOROLOGY AT THE BRITISH ASSOCIATION, BELFAST, SEPTEMBER, 1902.

The following extract from the opening address by Prof. Arthur Schuster, Chairman of the Subsection of Astronomy and Cosmical Physics, has so much that is of value to the meteorological student that by special request we reprint it from a recent number of *Nature*:

The question I wish to bring to your notice to-day is an old one: if two events happen simultaneously or one follows the other at a short interval of time, does this give us any reason to suppose that these two events are connected with each other, both being due to the same cause, or one being the cause of the other? Everyone admits that the simple concurrence of events proves nothing, but if the same combination recurs sufficiently often we may reasonably conclude that there is a real connection. The question to be decided in each case is what is "sufficient" and what is "reasonable." Here we must draw a distinction between experiment and observation. We often think it sufficient to repeat an experiment three or four times to establish a certain fact, but with meteorological observations the case is different, and it would, *e. g.*, prove very little if on four successive full moons the rainfall had been exceptionally high or exceptionally low. The cause of the difference lies in the fact that in an experiment we can control to a great extent all the circumstances on which the result depends, and we are generally right in assuming that an experiment which gives a certain result on three successive days will do so always. But even this sometimes depends on the fact that the apparatus is not disturbed, and that the housemaid has not come in to dust the room. Here lies the difference. What is possible in a laboratory, though perhaps difficult, is not possible in the upper regions of the atmosphere, where some unseen hand has not made a clean sweep of some important condition.

When we can not control accessory circumstances we must eliminate them by properly combining the observations and increasing their number. The advantage does not lie altogether on the side of experiment, because the very identity of condition under which the experiment is performed gives rise to systematic errors, which nature eliminates for us in the observational sciences. In the latter also the great variety in the combinations which offer themselves allow us to apply the calculus of probability, so that in any conclusion we draw we can form an idea of the chance that we are wrong. Astronomers are in the habit of giving the value of the "probable error" in the publication of their observations. Meteorologists have not adopted this custom, and yet their science lends itself more readily than any other to the evaluation of the deviations from the mean result, on which the determination of the probable error depends. We look forward to the time when weather forecasts will be accompanied by a statement of the odds that the prediction will be fulfilled.

The calculation of the probability that any relationship we may trace in different phenomena indicates a real connection seems to me to be vital to the true progress of meteorology, and although I have on previous occasions (*Cambridge Phil. Trans.*, Vol. XVIII. p. 107) already drawn attention to this matter I should like once more to lay stress on it.

The particular case I wish to discuss (though the methods are not restricted to this case) is that in which one of the two series of events between which relationship is to be established has a definite period, and it is desired to investigate the evidence of an equal period in the other series.

Connections between the moon and earthquakes, or between sun spots and rainfall if proved to exist, would form examples of such relationships. The question to be decided in these cases would be, is there a lunar period of earthquakes, or an 11-year sun-spot period of rainfall.

Everyone familiar with Fourier's analysis knows that there is a lunar or sun spot, or any other period in any set of events from volcanic eruptions down to the birthrate of mice; what we want to find out is whether the periodicity indicates a real connection or not. Let us put the problem into its simplest form. Take n balls, and by some mechanism allow them to drop so that each falls into one of m compartments. If finally they are equally distributed each compartment would hold n/m balls. If this is not the case we may wish to find out whether the observed inequality is sufficient to indicate any preference for one compartment or how far it is compatible with equality of chance for each. If we were able to repeat the experiment as often as we like we should have no difficulty in deciding between the two cases, because in the long run the

average number received by each compartment would indicate more and more closely the extent of bias which the dropping mechanism might possess. But we are supposed to be confined to a single trial, and draw our conclusions as far as we can from it.

It would be easy to calculate the probability that the number of balls in any one compartment should exceed a given number, but in order to make this investigation applicable to the general problem of periodicities we must proceed in a different manner. If the compartments are numbered, it does not matter in which order, and a curve be drawn in the usual manner representing the connection between the compartments and the number of balls in each, we may, by Fourier's analysis, express the result by means of periodic functions. The amplitude of each period

can be shown on the average to be $\frac{1}{m} \sqrt{\pi n}$. It is often more convenient

to take the square of the amplitude—call it the intensity—as a test, and we may then say that the "expectancy" of the intensity is $4n/m^2$. The probability that the intensity of any period should be k times its average or expectancy is e^{-k} . We may apply this result to test the reality of a number of coincidences in periods which have been suspected. A lunar effect on earthquakes is in itself not improbable, as we may imagine the final catastrophe to be started by some tidal deformation of the earth's crust. The occurrence of more than 7,000 earthquakes in Japan has been carefully tabulated by Mr. Knott according to lunar hours, who found the Fourier coefficient for the lunar day and its first three submultiples to be 10.3, 17.9, 10.9, 39.7; the expectancy on the hypothesis of chance distribution for these coefficients I find to be 19.3, 15.7, 10.6, 5.02. The comparison of their numbers disproves the supposed connection; on the other hand, the investigations of Mr. Davison on solar influence have led to a result much in favour of such influence, the amplitude found being in one series of observations equal to five times, and in the other to fifteen times the expectancy. The probability that so large an amplitude is due to accident in the first case is one in 300 millions, and in the second the probability of chance coincidence would be represented by a fraction, which would contain a number of over seventy figures in the denominator. We may therefore take it to be established that the frequency of earthquakes depends on the time of year, being greater in winter than in summer. With not quite the same amount of certainty, but still with considerable probability, it has also been shown that earthquake shocks show a preference for the hours between 9 a. m. and noon.

A great advantage of the scientific treatment of periodical occurrences lies in the fact that we may determine *a priori* how many events it is necessary to take into account in order to prove an effect of given magnitude. Let us agree, for instance, that we are satisfied with a probability of a million to one as giving us reasonable security against a chance coincidence. Let there be a periodic effect of such a nature that the ratio of the occurrence at the time of maximum to that at the time of minimum shall on the average be as $1+\lambda$ to $1-\lambda$, then the number of observations necessary to establish such an effect is given by the equation $n=200/\lambda^2$. If there are 2 per cent more occurrences at the time of maximum than at the time of minimum $\lambda=0.01$, and n is equal to two million. If the effect is 5 per cent, the number of events required to establish it is 80,000.

To illustrate these results further, I take as a second example a suggested connection between the occurrence of thunderstorms and the relative position of sun and moon. Among the various statistical investigations which have been made on this point, that of Mr. MacDowall lends itself most easily to treatment by the theory of probability. One hundred and eighty-two thunderstorms observed at Greenwich during a period of fourteen years have been plotted by Mr. MacDowall as distributed through the different phases of the moon, and seem to show a striking connection. I have calculated the principal Fourier coefficient from the data supplied, and find that it indicates a lunar periodicity giving for the ratio of the number of thunderstorms near new moon to that near full moon the fraction 8.17 to 4.83.

This apparently indicates a very strong effect, but the inequality is only twice as great as that we should expect if thunderstorms were distributed quite at random over the month, and the probability of a true connection is only about 20 to 1. No decisive conclusions can be founded on this, the number of thunderstorms taken into account being far too small. We might dismiss as equally inconclusive most of the other researches published on the subject were it not for a remarkable agreement among them, that a larger number of storms occur near new moon than near full moon.

I have put together in the following table the results of all investigations that are known to me; following the example of Koeppen, I have placed in parallel columns the number of thunderstorms which have occurred during the fortnight including new moon, and the first quarter and the fortnight including the other two phases.

It will be seen that out of fourteen comparisons, thirteen show higher numbers in the first column, there being also, except in two cases, a general agreement as regards the magnitude of the effect. Two of the stations given in the table, Göttingen and Gotha, are perhaps geographically too near together to be treated as independent stations, and we may therefore say that there are thirteen cases of agreement, against which there is only one published investigation (Schiaparelli) in which the maximum effect is near full moon.